

LETTERS TO THE EDITOR

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Nomenclature and Notation in Calorimetry.

ALL who are engaged in thermal investigations themselves, as well as those who have occasion to study the published work in this department of science, must have been frequently annoyed by the use of the word *calorie* with its varying signification. It has been sought to remove the inconvenience by qualifying the calorie as small or great, and in other ways; but on opening a book at any place where the results of thermal determinations are given, it is in most cases difficult to discover at once what unit of heat the author is using.

As different classes of investigation are carried on on different scales, it is obvious that it is a convenience, if not a necessity, to have different heat units at disposal. The unit which is suitable to express the thermal changes in a beaker in the laboratory, would manifestly be inconvenient when dealing with the daily or seasonal changes in a lake or an ocean. It is therefore natural and necessary to have heat units of different magnitudes, but it is neither natural nor necessary to call them all by the same name, and it is extremely inconvenient not to have a short form of notation which will show on its face the actual heat unit used.

In the early literature of the equivalence of heat and work in this country, one unit of heat is universally used; it is the pound-degree-Fahrenheit, and in the writings of Joule, Thomson, Rankine and others of that time, it is simply called "heat unit," as there was no other competing with it. With the rise and development of thermal chemistry, it was necessary to fashion the compound unit out of the simple units in common use in chemical laboratories; these are the gramme and the Celsius degree.

The heat given out by one gramme of water cooling by 1°C. at ordinary temperatures, is the unit most used in such researches; and it received the name of calorie, sometimes now called small calorie.

For many purposes this unit proved itself inconveniently small, and several larger units have been used, such as the heat given out by one kilogramme of water cooling 1°C. at ordinary temperatures, or the heat given out by one gramme of water cooling from 100°C. to 0°C. ; but the name of calorie was retained in connection with them all, and in the specification of a quantity of heat by a number, the nature of the unit was indicated by the syllable *cal.* or the letter *K*, neither of which, of itself gives any information.

In my own work, and in the study of the writings of others, I have adopted a form of notation which I have found so useful that I propose to lay it before the readers of NATURE. I do not doubt that others who interest themselves in calorimetric work have been driven to adopt some similar, perhaps the same, perhaps a better form of notation; and I think they will agree with me that some system of self-interpreting notation should be universally adopted without loss of time.

Just as, when dealing with work, we use currently the expressions foot-pound and kilogramme-metre, so in calorimetry it is quite common to talk of a gramme-degree, or a kilogramme-degree; and what I propose is to use no other expression than these compound and self-explaining ones, and, in writing, to express them shortly by g° and k° respectively, to which for clearness the symbol of the thermometric scale must be added, so that they become $g^{\circ}\text{C.}$ and $k^{\circ}\text{C.}$ when Celsius' scale is used, or $g^{\circ}\text{F.}$ and $k^{\circ}\text{F.}$ when Fahrenheit's scale is used.

On this system the expression $g^{\circ}\text{C.}$ would replace the ordinary "cal." and Ostwald's *K* would be represented by $100\text{ }g^{\circ}\text{C.}$ or $0\cdot1\text{ }k^{\circ}\text{C.}$, or by $h^{\circ}\text{C.}$ to mean hectogramme-degree *C.* With perfect exactness *K* would be expressed by $g\text{ }100^{\circ}\text{C.}$, but the difference between $100\text{ }g^{\circ}\text{C.}$ and $g\text{ }100^{\circ}\text{C.}$ is much less than the probable experimental error in any calorimetric operation. In a table containing a column of quantities of heat expressed in numbers of gramme-degrees-Celsius, the nature of the unit would be indicated at the top of the column by $g^{\circ}\text{C.}$; exactly as, in a column of temperatures, the unit is indicated by the symbol $^{\circ}\text{C.}$ or $^{\circ}\text{F.}$ The original British heat unit is then clearly expressed by $\text{lb.}^{\circ}\text{F.}$

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A heat unit made up of any unit of weight and any unit of temperature can be perfectly expressed in this system. Thus, if there were any advantage in doing so, we might have $g^{\circ}\text{F.}$, $\text{lb.}^{\circ}\text{C.}$, $k^{\circ}\text{R.}$ and many others, and their meaning would be at once apparent on inspection.

In oceanographical work, where the heat exchanges between one layer of water and another, or between the water and the air are under discussion, I have found the most convenient heat unit to be the fathom-degree-Fahrenheit, or the metre-degree-Celsius, which are abbreviated for the purposes of notation into $f^{\circ}\text{F.}$ and $m^{\circ}\text{C.}$, respectively. The nature of this unit will be most easily understood by considering an example.

In a paper, "On the Distribution of Temperature in Loch Lomond in the Autumn of 1885," read before the Royal Society of Edinburgh, and published in its *Proceedings* for the session 1885-86, I have given, at page 420, a table of the changes in the distribution of heat in the direction of depth, between several pairs of dates, in the Luss basin of Loch Lomond. At a certain depth, indicated by the intersection of the temperature curves, the temperature of the water is the same on both dates. The season being autumn, the layer above this depth has been losing heat, partly to the air above and partly to the water beneath, while the layer below the depth of common temperature has been on the whole the gainer. Thus, taking the dates September 5 and October 15, the intersection of the temperature curves is found at a depth of 16 fathoms; and in the interval of forty days the mean temperature of the water above this depth has fallen by $5\cdot8^{\circ}\text{F.}$ from $55\cdot0^{\circ}\text{F.}$ to $49\cdot2^{\circ}\text{F.}$ The thickness of the layer is 16 fathoms; therefore the loss of heat has been $16 \times 5\cdot8 = 92\cdot8\text{ }f^{\circ}\text{F.}$, or $92\cdot8$ fathom-degrees-Fahrenheit. The total depth of the lake at the spot was 35 fathoms, therefore the layer of water below the depth of common temperature was 19 fathoms thick. The mean temperature of this layer was $47\cdot1^{\circ}\text{F.}$ on September 5, and $48\cdot9^{\circ}\text{F.}$ on October 15, showing a rise of $1\cdot8^{\circ}\text{F.}$ in the interval. This corresponds to a gain of heat represented by $19 \times 1\cdot8 = 34\cdot7\text{ }f^{\circ}\text{F.}$ Assuming that the heat gained by the lower layer has been entirely at the expense of the upper one, we see that the loss of heat of the upper layer, during the interval, has been to the extent of 37·4 per cent. to the deeper water, and 62·6 per cent. to the air. The upper layer of water has thus been passing heat at the average rate of $1\cdot485\text{ }f^{\circ}\text{F.}$ into the air, and into the deeper water at the rate of $0\cdot85\text{ }f^{\circ}\text{F.}$ per day.

It is worthy of remark that the fathom-degree-Fahrenheit and the metre-degree-Celsius are interchangeable in heat calculations, because the fathom is 1·8 metre and the Celsius degree is $1\cdot8^{\circ}\text{F.}$

This is a great convenience, and its usefulness will be apparent by applying it to the above example.

We have seen that, during the interval of forty days, the average transmission of heat from the upper layer of water has been at the daily rate of $1\cdot485\text{ }f^{\circ}\text{F.}$ to the air and of $0\cdot85\text{ }f^{\circ}\text{F.}$ to the deeper water. Writing $m^{\circ}\text{C.}$ for $f^{\circ}\text{F.}$, and considering a horizontal area of one square centimetre, we find at once that the average daily supply of heat from the water to the air has been at the rate of $148\cdot5\text{ }g^{\circ}\text{C.}$, and to the deeper water at the rate of $85\text{ }g^{\circ}\text{C.}$ (gramme-degrees-Celsius) per square centimetre of superficial area.

It is unnecessary to provide for special cases where specially suitable units will be chosen as a matter of course; but for ordinary work of constantly recurring type it is important to have a system of nomenclature and of notation, each of which will tell its own story.

J. Y. BUCHANAN.

May 4.

Future Rainfall.

MOST people probably suppose that we have no light whatever on the fluctuations of our rainfall in future years, and that he would be a bold meteorologist who offered to forecast them. Yet, if there be truth in the conclusions arrived at by Prof. Brückner, we are not wholly without light on the subject; for a part of this country, at least, in common with probably the greater part of the globe, is subject to a regular recurrence of cold and wet periods, at about 35 years intervals (measuring from the centre of one such period to that of the next); these periods alternating with others which are hot and dry. It seems useful to inquire how we at present stand, and, if possible, what are our present prospects in respect of this theory.

For this purpose I will here employ a simple meteorological method, which seems to have been little used among us hitherto, viz. algebraic addition, step by step, of a series of plus and minus values; the resulting figures being then plotted as a curve.

Suppose e.g. any set of such values, as follows:

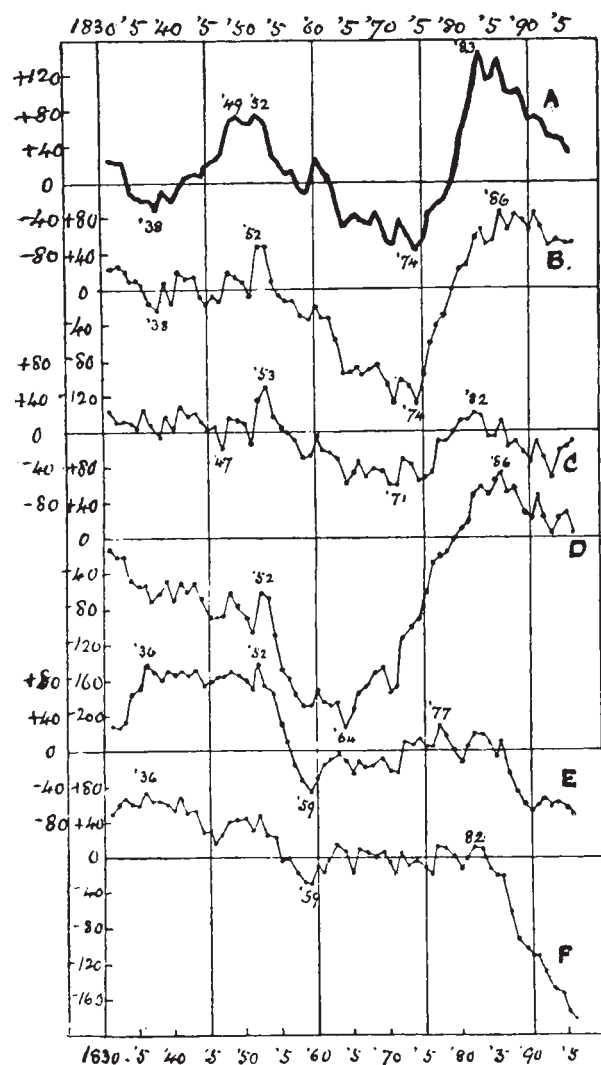
$$+3 + 6 - 2 + 8 + 12 - 3 - 9, \&c.$$

By addition we get this series:

$$+9 + 7 + 15 + 27 + 24 + 15, \&c.$$

This latter series is thrown into a curve.

In his *British Rainfall* for 1881, 1891, &c., Mr. Symons has given a series of rainfall values for a number of stations in



A, Boston; B, Oxford; C, Chilgrove; D, Exeter; E, Kendal; F, Bolton.

different parts of England (from 1830 onwards); also the percentage equivalents of these, the average for each station being taken as 100. These latter I have made use of, taking the excess over 100 as a plus value, and the deficiency under 100 as a minus. Thus e.g. 106 would be +6; 94 would be -6.

When this has been done with the values for Boston, in Lincolnshire, and the series treated by the addition-method described, we get the curve marked A in the diagram; and it is to this curve I would especially invite attention. For it is to the eastern parts of our country that Brückner's cycle applies; the west belongs to what he calls *Ausnahmegebiete*, or exceptional regions.

Bearing in mind that these curves rise for plus values and fall

for minus, we note in this Boston curve a general rise from 1838 to 1849 (or 1852), also from 1874 to 1883; while we have general fall from 1852 to 1874, and from 1883 to 1896 (the last point dealt with). In other words, the two former were *wet* periods, the two latter *dry* periods.

The following figures show this:

	Years.	Dry.	Wet.
1838-52	14	4	10
1874-83	9	none	all
1852-74	22	17	5
1883-96	13	9	4

Between the wave crests 1849 and 1883 are 34 years; and between the wave hollows 1838 and 1874 are 36 years. Or, if we like to take as approximate centres of the wet periods (say) 1843 and 1878, we have about the same interval, 35 years.

These fluctuations, standing alone, would clearly be too slender evidence of a cycle. But Brückner claims that his cycle of 35 years has been in evidence in various parts of the globe through these two centuries at least (since 1700).¹ If the eastern part of England, then, may be expected to conform to the law in future, we might reasonably, perhaps, look for the centre of another wet period somewhere in the second decade of next century. And for the near future (without attempting detail) a continuation for some years of the recent *régime* seems not unlikely, dry years preponderating over wet. That is, the curve should go down further, on the whole, for some years yet.

I have given a number of other curves for comparison, viz. B, Oxford; C, Chilgrove, in Sussex (near Chichester); D, Exeter, less reliable perhaps (see *Brit. Rf.*, 1881); E, Kendal; and F, Bolton, in Lancashire. As we go westwards, the curve seems to degrade somewhat (regarded from our standpoint), though the same type may be discerned. In the northern curves, Kendal and Bolton, we find little in common with the Boston curve, though something like a 35 years interval may, in cases, be made out.

It may be well to state that other stations in the east of England yield very similar curves to that for Boston.

A. B. M.

Prehistoric Egypt.

I NOTICE a review in NATURE containing some statements about my own work which are inaccurate.

The term "New Race" is quite correct, as the race was entirely new to us, whatever their age; and as a tentative name which commits us to no theories, it can hardly be said that I "did not understand the facts of the case" in using it. In dating the race to at least 3000 B.C., I was doing all that the facts warranted at the time; and if we all agree now that they are older, it is by mere consensus of guessing, for no absolute proof of earlier age by juxtaposition with other things has even yet been found.

Further, Dr. Verneau's erroneous assumption that the condition of the bones could be produced by exposure to the air alone, is quoted, with the remark that my "sensational discovery therefore falls to the ground." How exposure to the air can possibly break off the ends of bones and scoop out the cellular structure, while the bone remains hard and firm, not even Dr. Verneau can explain. To deny cannibalism in such a case, reminds one of the indignant repudiation of the intention to smoke by a man who already has a pipe in his mouth and a match in his fingers.

No one values more than I do the discoveries of M. de Morgan; but had he dealt more with strict evidence and full details, and given weight to many facts which he has ignored, I venture to think that his work would have needed less revision in future.

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I HAVE read Mr. Petrie's letter, and I still think that M. de Morgan is right, and that Mr. Petrie is wrong. I also think that Dr. Verneau is right, and that his "assumption" is not "erroneous." Mr. Petrie's reference to the revision which he thinks M. de Morgan's work will need is remarkable; for his own will—in my opinion—need much more! however much M. de Morgan's may need!

THE REVIEWER.

¹ We may here recall that Bacon seems to have been aware of such cycle.